REVIEWS.

ART. XII.—*Physiological Chemistry.* By Professor C. G. LEHMANN. Translated from the second edition by GEORGE E. DAY, M. D., F. R. S., Fellow of the Royal College of Physicians, and Professor of Medicine in the University of St. Andrews. Edited by R. E. ROGERS, M. D., Professor of Chemistry in the Medical Department of the University of Pennsylvania. With Illustrations, selected from *Funke's Atlas of Physiological Chemistry*, and an Appendix of Plates. 2 vols. 8vo., pp. 648 and 547. Philadelphia: Blanchard & Lea. 1855.

THERE is no department of Physiology that promises larger rewards to a laborious and judicious investigation than that which is concerned with the chemical composition of the solids and fluids of the body, and the chemical changes that take place in the recesses of the animal organization. While the enlightened physiologist studies, with nearly equal interest, the chemical, mechanical, and nervous phenomena characteristic of vital processes, and regards them as equally expressive of the peculiar character that distinguishes living organisms, it cannot be denied that the chemical portion of these studies is a most inviting one. Notwithstanding, however, its promising aspect, and the abundant labour which has been bestowed upon it, its results have thus far failed to satisfy our expectations. Many brilliant chemico-physiological doctrines, which have grown up from time to time, have proved of but little permanent value; and though the study of physiological chemistry has not been by any means without profit, its results certainly have not corresponded to the labour and genius which have been bestowed on them

One great cause of this misfortune lay in a mistake as to the true relation between chemistry and physiology, and the previous acquirements necessary to enable any one to prosecute the study with advantage. Chemistry, with its symbolical language and magical effects, still retains, in the minds of those unacquainted with its details, something of the old character of mystery with which the alchemists surrounded it. Instead of regarding it simply as the study of the properties and phenomena presented by matter under different external conditions, they are apt to look upon it as a sort of hocus-pocus, by which obscure questions are to be answered in some inexplicable manner. When the physician, therefore, first felt the need of a more complete knowledge of the processes of health and disease than he could gain from ordinary anatomical and clinical sources, he applied for assistance to the professed chemist, very much as he might apply to a fortune-teller. Knowing, himself, next to nothing of chemistry, he hoped to use it as a divining-rod, which should discover hidden sources of knowledge, and enable him to arrive, by an easy path, at results hitherto unattainable. On the other hand, the chemist knew nothing of anatomy and physiology. He analyzed the healthy tissues and morbid products of the body, just as he would the inorganic substances upon which his labours had previously been bestowed. He ascertained their elementary or ultimate composition, and investigated the changes which they suffered by his acids and his

alkalies, his burning and his destructive distillation. These were the only points which a purely chemical examination could elucidate; and supposing that such results were sufficient for the requirements of physiology, he applied his conclusions to the explanation of the vital processes in health and disease, disregarding entirely the peculiarities of the animal organization, and the special conditions under which its chemical phenomena originate. These conclusions were accepted by the physician, and constituted, in many instances, the basis of his physiological and pathological theories. It is not, therefore, surprising that doctrines so ill-founded should be rapidly replaced, and that labours so injudiciously directed should produce but few valuable results.

It has gradually become manifest, however, that what is called "Physiological Chemistry," that is, the study of the chemical changes taking place in the body, is really a part of physiology, and not at all of pure chemistry; and that if a previous chemical education, and a familiarity with chemical manipulation be necessary to the physiologist who undertakes it, the chemist has at least an equal need of comprehending the details of anatomy and the general character of physiological phenomena. The knowledge which we require in order to advance us in this important elementary branch of medicine, is not what changes the animal tissues and fluids undergo in the flask and the crucible, but what changes they undergo in the living body; and so long as our investigations in this direction were prosecuted by men who were not familiar with the peculiarities of its structure, they naturally remained sterile and unsatisfactory.

The extent and variety of Professor Lehmann's acquirements make him better qualified to undertake the study of physiological chemistry than most of those who have preceded him. Educated as a professed chemist, he is yet familiar with the details of both ordinary and minute anatomy, and understands evidently the practical requirements of physiological experiment. He starts, therefore, with a decided advantage over his predecessors, and makes, consequently, a greater progress in the end. He takes a physiological, and not a purely chemical view of the question, and treats the subject, if not with the severe and logical accuracy of the French, yet in an exceedingly comprehensive and satisfactory manner.

In his introductory chapter, he exposes the errors of judgment which have hitherto retarded the progress of physiological and pathological chemistry. Perhaps the most prevalent of these, and at the same time the most injurious in its effects, was the practice, which was early adopted, of regarding the *elementary composition* of organic substances as the point of principal importance in the investigation; and of using this elementary composition as a basis upon which to construct, by a sort of chemical divination, a theory of the molecular changes taking place in the body. Thus, chemists, having ascertained the ultimate constitution of albumen, fibrin, the fats, sugars, etc., instead of laboriously tracking these substances in their course though the body, and searching for their mode of origin and their metamorphoses by direct observation, did not hesitate to supply from the imagination what was wanting to experiment; and by moving about on paper their atoms of oxygen, hydrogen, carbon, and nitrogen, like chessmen on a chessboard, showed how ingeniously urea could be constructed out of fibrin, and carbonic acid out of oil and sugar. These conclusions, however, were unfortunately quite destitute of any foundation in nature. The changes which they expressed existed in the imagination of the chemist, but no evidence was adduced that they actually took place in the interior of the body. Such a mode of explaining vital phenomena was not

only valueless in itself, but positively injurious, by falsely satisfying the mind of the inquirer, and so shutting up the pathway to more profitable investigation. The entirely baseless character of its results is well exhibited by Lehmann in the following remarks:—

> "Physiological chemistry has given rise," he says, vol. i. p. 19, "to many delusions of this nature, owing to its imperfect development, and to the necessity presented by physiology and pathology for chemical elucidation. Some few isolated deductions were drawn from superficial chemical experiments, and arranged in a purely imaginary connection by the aid of symbols and formulæ, for whose establishment analysis, in many cases, did not even afford any sanction. Thus, for instance, in the attempt to form a conclusion regarding the metamorphosis of the blood from an elementary analysis of its solid residue, and of the composition of the individual constituents of the excretions, there is an utter absence of all scientific groundwork; for, independently of the fact that the elementary analysis of so compound a matter as the blood is incapable of yielding any reliable results, and cannot, therefore, justify the adoption of any special chemical formula, it is assuredly most illogical to attempt to compare the composition of the blood collectively with that of the separate excrementitious matters. In such deductions, expressed by chemical formulæ, the addition of atoms of oxygen, and the subtraction of those of water, carbonic acid, and ammonia, are wholly arbitrary; for chemical analyses do not afford the slightest grounds for the majority of these equations. * * * Chemical equations, having no other foundation than the presumed infallibility of empirical formulæ, must, however, cause us to deviate from the path of physical inquiry, and involve us in a chaos of the most untenable delusions. Thus, for instance, a chemical equation might lead us to conclude that glycine (glycocol) was the source of urea and lactic acid in the metamorphosis of the animal tissues; for we might conclude that two equivalents of hydrate of glycine were decomposed into the above named substances according to the formula $C^8H^{10}N^2O^8 = C^2H^4O^2N^2 + C^6H^5O^5.HO$. All the experiments hitherto instituted with glycine are nevertheless opposed to such a disintegration. If, then, we would deduce urea and lactic acid from glycine, which has not been proved to exist in the blood, we should be neglecting the most comprehensive rule of logic, according to which, one hypothesis cannot be supported by another. It has, however, unfortunately been too much the practice, in recent times, to employ far more complicated equations as supports for such purely subjective modes of contemplation, by which a semblance of the most exact methods of investigation has been assumed. By these means, a number of chemical fictions have supplanted the fancies of that speculative natural philosophy which in earlier times encumbered the study of physiology and pathology, and have plunged medicine into the midst of a new labyrinth of untenable theories."

Another serious error of previous investigators has been their *disregard*, already alluded to, *of anatomical and physiological conditions.* The fluids, for example, are chemically analyzed, without any clear idea of their anatomical constitution, in such a manner that the most heterogeneous ingredients are arranged side by side, without any account being taken of the essential differences between them. In quantitative analysis of the blood we find, over and over again, the percentage of the blood-globules, which are morphological elements, themselves containing globulin, hematin, fats, etc., estimated in company with that of sulphuric acid, a substance which does not exist at all as such in the blood, but is artificially produced by the decomposition of its sulphates. There are many recorded analyses of the blood, which have evidently been conducted, so far as regards chemical manipulation, with much labour and accuracy, in which we are at a loss to conjecture from what part of the vascular system the specimen was taken. Again, if the blood analyzed were taken from the portal vein, for example, it is not mentioned whether it were taken

before or after feeding, or, in the latter case, what had been the quantity and quality of the food. The practical physiologist will at once understand that such deficiencies as these must make the analyses entirely valueless for any purposes of comparison.

In examinations of the morbid tissues and fluids, the particular condition of the patient is imperfectly or erroneously described; and yet an accurate knowledge of this particular condition is certainly necessary to give the analysis any scientific or practical value.

A third source of misconception and wasted labour, in physiological chemistry, consists in overlooking the deficiencies and imperfections of the science in its present condition. We endeavour to establish the relation between two different organic substances, or the connection between two different phenomena, before the substances or the phenomena themselves are thoroughly known to us. The incompleteness of our knowledge with regard to the relative quantity of the different ingredients of the blood, for example, and the practical difficulty of separating them by analysis, are too often disregarded by us, in our haste to grasp at secondary results.

After exposing in detail all these sources of error, and cautioning his readers against them, the author indicates three different methods by which investigations in physiological chemistry are to be carried on, each of which has its own special use, and is applicable to particular questions. The first is what he calls the *statistical* method. It consists in comparing the final products of the animal organism with the substances consumed—the egesta with the ingesta—without endeavouring to establish the intermediate phenomena. This plan, which is limited in its application, is, nevertheless, of great value for some purposes.

> "Some of the most important results" (vol. i. p. 27), "whose solution was specially necessary, were unanswerable by any other method. Thus, for instance, it was ascertained, by an accurate investigation of the food, and its comparison with the constituents of the excreta and of the nutrient fluids, that in the ordinary food of animals, albuminous substances occur in sufficient quantity to compensate for the nitrogenous matters lost in the process of nutrition and in the metamorphosis of tissue; while it was thus, at the same time, shown that the animal organism does not necessarily possess the property of generating albuminous matter from other substances containing nitrogen."

The second method is the *chemico-experimental.* It consists in operating upon the animal substances by the various chemical reagents, the natural conditions of the substance in the body being imitated as closely as possible, and endeavouring to ascertain by inference the possible or probable changes which such substance undergoes in the vital process. Thus the fats are decomposed into glycerine and saponaceous bodies, or give rise to the fatty acids; glykocholate of soda yields glycine, cholic acid, choloidic acid, etc.; and the albuminoid substances leucine, or the various products of combustion. This is the method which has been so much abused by chemists, and which is constantly liable to abuse, from the fact that we can never exactly imitate the physiological conditions by an artificial contrivance, and sometimes this imitation is either altogether impracticable, or is totally neglected. Properly employed, this method may be of service to us by pointing out the direction in which our subsequent researches may profitably be made, but can never supersede the necessity of making these researches. Thus the artificial production of cholic and choloidic acids from glykocholate of soda indicates the possible mode of decomposition of the bile in the intestine, and suggests the search for those substances in the intestinal contents; but it does not give any de-

cisive information, and should always be employed with a clear idea of its true object and limits.

The third method, the most important of all, is the *physiologico-experimental.* This is the method by which the operations of nature are directly observed in the living body itself. A previous acquaintance with the chemical constitution of the different ingredients of the frame is, of course, necessary, and a knowledge of their reactions under artificial conditions highly favourable to success in this line of study. The different ingredients of the food are then followed through the intestinal tract, and their changes and mode of disappearance ascertained. The alterations of the blood in different parts of the system, and under different conditions of fasting and feeding, rest and activity, etc., are similarly investigated. Lehmann indicates fully the value of this method as follows:—

"We are aware," he says (vol. i. p. 29), "that we shall never succeed in artificially reproducing all the processes as they occur in the living body, since we are here as little able to call forth the accessary conditions and relations, as in the formation of minerals and rocks. It is, therefore, the more necessary to observe a process, of which we cannot judge by imitation, in its course in the living body; and for this end we must chiefly employ natural physiological means. Among these we may reckon the investigations that have been made in reference to the contents of the stomach during the process of natural digestion, to the chemical change of individual substances in the development of the egg during incubation, and to the dependence of the products of respiration on different external conditions. We may further add those experiments that have been made on the changes of individual substances during their passage through the animal organism, or on the effect of different kinds of food, and the metamorphoses of certain nutrient substances during the process of nutrition. To the same method belong all pathologico-chemical experiments, as, for instance, observations on the contents of the intestine after the closure of the common bile duct, and on the blood and other fluids after extirpating or tying the vessels of the kidneys. Chemistry, unfortunately, too often fails us, to permit of our deriving from this method all the results which it appears to promise; it must, however, ultimately furnish the keystone to all physiologico-chemical inquiries, which, without its aid, would continue insoluble enigmas, and would admit of hypothetical, rather than actual explanation."

The plan adopted by Lehmann in the body of the work is to divide his subject into three principal sections. The first division embraces the study of the ORGANIC SUBSTRATA of the body; that is, of the individual substances entering into its composititition, such as albumen, fat, saline matters, etc. The author takes up in succession—

1. *The Non-nitrogenous Acids,* viz: Oxalic, acetic, butyric, lactic, margaric, benzoic, lithofellic acids, etc.
2. *The Nitrogenous Bases:* Creatine, creatinine, urea, glycine, leucine, guanine, etc.
3. *The Conjugated Acids;* or, nitrogenous bodies, of complicated composition, which may be hypothetically regarded as composed of a non-nitrogenous acid, modified by union with a neutral organic substance, as uric, glycocholic, and taurocholic acids.
4. *Haloid Bases and Salts:* Glycerine, fats, etc.
5. *Lipoids:* Cholesterin, serolin, castorin, etc.
6. *Non-nitrogenous neutral bodies:* Glucose, cellulose, milk sugar, etc.
7. *Colouring matters:* Hematine, melanine, colouring matters of bile and urine.
8. *Extractive matters.*

9. *Histogenetic substances:* Albumen, fibrin, casein, etc.
10. *The Mineral Constituents of the body:* Chloride of sodium, iron, the alkaline phosphates and sulphates, etc.

This first division may be regarded as almost strictly chemical in its character. Entirely subordinate to the general scope of the work, its object is to render us familiar with the properties of the different ingredients of the body, so that they be properly distinguished from each other, and recognized with certainty during the course of subsequent investigation.

The second division is more anatomical in its plan, and comprises the examination of the ANIMAL JUICES and of the SOLID TISSUES; that is, it treats of the animal substances, not as isolated and purified by manipulation, but as they occur naturally mingled together in certain proportions, so as to make up the different tissues and fluids of the body. The proportionate quantity of each individual element or "organic substratum," and its mode of union with the rest, lead naturally to a consideration of the properties of the animal fluid or tissue as a whole. When, as for example in the blood and solid tissues, morphological elements occur, Lehmann gives a description of these, and carefully distinguishes their different parts in his analysis. He is careful, also, to indicate the proper mode of obtaining the fluids for examination, a point which has been too often neglected, but which certainly cannot be overlooked without producing serious inaccuracy in the result.

"The result of the whole chemical operation," he says (vol. i. p. 407), "is often dependent on the manner in which the object is exhibited, and it will be found that an unsuitable method of exhibition frequently leads to the adoption of wholly erroneous views in reference to the nature and function of an animal fluid. It is only when we are convinced that the animal fluid is presented to us in the same state in which it exists in the living body, that we can hope to obtain any physiological result from our investigation. As the exhibition of many animal fluids, moreover, frequently requires that the experimentalist should be familiar with certain anatomico-surgical operations, we think it will hardly be deemed superfluous if we consider the methods adopted for procuring some of the animal juices."

In treating of the *saliva*, Lehmann adopts the conclusion that the secretion acts in the digestion of the food rather by its physical properties, by simply moistening and lubricating the mass, than by any chemical action exerted on the alimentary substances. One of the most interesting points, in regard to this secretion, is its property of converting starch into sugar when artificially digested with it at the temperature of 100° F. The history of opinions on this point affords a very striking instance of the importance, in any physiological investigation, of referring constantly to the living body, as the only source of positive and complete information. Even when we can obtain the animal fluids in a fresh and normal condition, the properties and reactions which they manifest in the flask or the test-tube, do not positively indicate their real action in the vital processes, but only suggest probabilities, which can be ultimately tested in the interior of the organism alone. When the discovery was first made that the saliva had the remarkable property of converting boiled starch into sugar in the course of ten or fifteen minutes, it was at once accepted as an entire explanation of the physiological destination of the saliva, and also of the method by which the digestion of starch was accomplished. The French, however, soon found that though this action was very constant when starch and saliva were artificially mixed, it did not take place in the stomach of the living animal; and though the Germans at first

asserted that the metamorphosis of starch did really continue in the stomach, later examinations have shown that the statements of the French were correct. We have frequently convinced ourselves, that if dogs with gastric fistulæ be fed on a mixture of meat and boiled starch, no sugar is to be found afterward in the fluids drawn from the stomach. While the meat remains in the stomach, the starch rapidly passes into the intestine, where it is at once converted into sugar, and where an abundance of sugar may be found if the animal be killed a short time after feeding. In this case, the conversion of starch into sugar, which would otherwise be accomplished by the saliva, is checked by the presence of the gastric juice, and does not recommence until the starch has passed out of the stomach, and comes in contact with the intestinal fluids. When starch is taken as a natural ingredient of the food, it is always mixed with albuminous matters, and therefore meets with gastric juice in the stomach. The earlier and erroneous conclusion of the Germans, with regard to this point, were very probably owing to their feeding the fasting animal on pure starch, which, not exciting the gastric secretion, underwent a partial saccharine conversion in the stomach. This, however, did not represent the natural conditions of the digestive process, and the point was afterwards abandoned.

A little difficulty may, perhaps, arise for the reader of the present volumes, from the fact that the English translator appended to his translation of the second edition, published in 1849, '50, and '51, an appendix, taken from the third edition, published in 1853; and the American editor has incorporated these additions into the main body of the text. This is a more convenient arrangement than the other, but occasionally liable to produce some confusion. In the present instance, for example, Lehmann appears to express two contradictory opinions within three or four pages of each other.

"And hence," he says (vol. i. p. 431), "if the conversion of amylaceous matter into sugar be the physiological function of the saliva, its action must not be confined to the short time in which the food remains in the mouth, but must also be continued in the stomach and intestines. Now, we may readily convince ourselves that this is really the case, by observing what occurs in an animal in whom a gastric fistula has been established; for while pure gastric juice exerts no action on starch, sugar may be detected by the ordinary means, in the stomach of the animal, in ten or fifteen minutes after it has swallowed balls of starch, or after they have been introduced through the fistula. Hence, it cannot be doubted that the saliva, after it has been mixed with the other animal secretions, continues to exert its action on the amylacea in the digestive canal."

Further on, at page 434, he says:—

"Bidder and Schmidt, under whose superintendence the experiments of Jacubowitsch were instituted, have convinced themselves, by later experiments, that the saliva loses its action on boiled starch in the stomach of the living animal." * * * "We are consequently led, by the earlier observations of Bernard, as well as by the more recent investigations of Bidder and Schmidt, to the conclusion, that notwithstanding its energetic action on starch, and notwithstanding its abundant supply, the saliva takes no very important part in the digestion of the amylacea. Hence, its principal use in the animal body must be of a mechanical nature. Beside the uses of this nature, mentioned in the text, Bidder and Schmidt believe that one of the main objects of the salivary secretion is its co-operation in the perpetual interchange of the watery fluids within the living organism."

The former of these extracts belongs to the second of Lehmann's editions, the latter to the third.

In the chapter on the *Bile*, abundant references are made to the elaborate experiments of Bidder and Schmidt, as well as to those of other observers. The proximate constitution of this refractory secretion seems at last to be tolerably well understood. It is a solution of the taurocholate or glykocholate of soda, or of both, according to the species of animal. Beside these characteristic ingredients, there are cholesterin, fats, fatty acids, pigment, chlorides, phosphates, and alkaline carbonates. With regard to the importance of the bile in the digestive process, Lehmann comes to the conclusion, principally from the results obtained by Bidder and Schmidt, that it is, to some extent, useful in dissolving the fats of the food; but that, on the whole, its agency as a digestive fluid is secondary and unimportant.

> "Moreover," he says (vol. i. page 497) "the experiments made on animals in which fistulous openings were established between the gall-bladder and the external abdominal walls (by which means all the bile that was secreted escaped externally), which have led Schwann, Blondlot, H. Nasse, and Bidder and Schmidt to very opposite views, do not prove that the bile exerts any *very* great influence on the digestive process. If animals can live for two or three months, or even half a year, without the passage of bile into the intestinal canal, the function of this fluid in digestion must at all events be a very limited, and probably only an indirect one, and this is the conclusion we should draw from the accurate and ingeniously devised experiments of Bidder and Schmidt; for, as has been already mentioned, the secretion of bile does not attain its maximum till the tenth hour after food has been taken, and by this time by far the greatest part of the ingesta has passed along the duodenum; hence the bile enters the small intestine at much too late a period to exert in it any great influence on the metamorphosis of the chyme. The biliary secretion unquestionably stands in a definite relation to digestion; a relation, however, which must be considered rather in the light of an effect or consequence of the digestive process than as an intermediate link in the process itself."

These conclusions must recommend themselves to all who are acquainted with the present state of our knowledge concerning the bile. Still, they are negative rather than positive, and, so far, are unsatisfactory. The knotty question still remains, "*What is the bile meant for?*" We cannot at all acquiesce on this point in the opinion expressed by Lehmann, according to which the most important function of the liver is the "*formation*, or at all events, the *rejuvenescence of the blood-corpuscles*," and the biliary constituents accessory products of this formation or rejuvenescence. The opinion that the blood-corpuscles perish bodily in any part of the circulation, and are replaced by new globules of fresh formation, is entirely destitute of experimental evidence. There is no reason for considering the blood-globules as less permanent than the fibres of muscle, or any other anatomical element; and though they must, like every other constituent part of the organism, be undergoing a constant molecular renovation, yet the products of this molecular change are certainly not the ingredients of the bile; for these substances—taurocholate and glykocholate of soda—are formed, as every investigation goes to show, solely in the liver, and not in any other part of the organism. There is another circumstance, however, connected with the ultimate destiny of the secretion, which is spoken of by Lehmann as "comparatively unimportant," but which, on the contrary, appears to us as more deserving of attention than any which have yet been mentioned; and that is, the *reabsorption from the intestine of certain of its ingredients.* If this statement of Liebig be corroborated, as is highly probable, it will indicate at least the direction in which the biliary constituents are to be followed; and will show that, though not a digestive fluid, the bile is certainly destined for use

in some other part of the body, for which it is prepared by its passage through the intestine.

The *pancreatic juice* is regarded as principally important in completing the conversion of starch into sugar. The German writers generally refuse to admit Bernard's doctrine, that the pancreatic juice has for its object the digestion of the fatty matters; and, strangely enough, deny even that it forms a permanent emulsion when shaken up with oil in a test-tube. It is not easy to understand why such contradictory statements should be made in regard to a fact so easily observed.

The remainder of the second division is occupied with the constitution of the intestinal fluids, excrements, blood, urine, and the various solid tissues.

The third section of the book, which is strictly physiological in character, is devoted to a consideration of the ZOO-CHEMICAL PROCESSES, or the manner in which the "organic substrata," united with each other in the organic mixtures of the animal fluids and solids, are interchanged and modified in the course of the vital processes. The author takes up the Origin of Organic Matter in the Vegetable Kingdom, and passes in review the general characters of the Molecular Movements in the animal organism, with the special functions of Digestion, Respiration, and Nutrition. The chapter on *Respiration* is one of the most extended and carefully written in the book. It comprises the description of the changes induced in the air and the blood during respiration, the modifying influences of various external and internal conditions, and gives an exposition of the author's views on the theory of respiration and animal heat. When Lavoisier, after establishing the nature of the combustive process, discovered that oxygen disappeared from the inspired air, and was replaced in expiration by carbonic acid and water, chemists and physicians both leaped at once to the conclusion that the true theory of respiration was attained, and that the process was to be regarded as an actual combustion of carbon and hydrogen in the lungs, resulting in the evolution of heat and the production of carbonic acid and water, exactly as it would happen in the furnace or the combustion-tube. The simplicity of the theory recommended it to every one, and it was adopted, at first, almost without question. Subsequently, when it became necessary to modify it in some particulars, its essential character was still retained. It was soon found, for example, that this combustion certainly did not, as previously supposed, take place in the lungs, and it was therefore referred to the capillaries of the general system. Further, the quantity of oxygen absorbed being found to be greater than that returned under the form of carbonic acid and water, it was seen that a part, at least, of the inspired oxygen must be applied to other purposes than the combustion of carbon and hydrogen. Still, the fundamental idea remained, that animal heat was the result of direct oxidation of certain ingredients of the blood which had been absorbed for that purpose; and that the lungs served, at the same time, as the supply-pipe and chimney-flue to the animal furnace. It was, however, overlooked that the evidence of this direct combustion was altogether wanting. In fact, whenever attempts were made to localize it in the body, it always receded before the pursuit of the experimenter, until somewhat recently doubts began to arise whether it really had an existence anywhere, and whether the natural heat of the body were not produced by other chemical changes than those usually known under the name of combustion. In France, Robin and Verdeil have denied positively that this combustion of the blood was anything more than a chimera, and have argued the point in an exceedingly thorough and forcible manner. Leh-

mann's views are somewhat more moderate than theirs, but he, also, rejects as untenable the substance of the combustion theory, and retains only a portion of its phraseology.

After ascertaining that the blood, in passing through the lungs, absorbs oxygen and exhales carbonic acid, and that the carbonic acid is not produced by direct oxidation in the lungs, but is brought to them ready-formed in the blood, the next most important question that arises for solution is the following: *Whence does the blood derive its carbonic acid?* Lehmann shows conclusively that the origin of the carbonic acid is in the tissues themselves, and not in the blood.

"It is therefore probable," he says (vol. ii. p. 473), "that also in the higher animals, endowed with true blood, the carbonic acid is almost entirely formed in the functional organs, and not in the liquor sanguinis. And should we then find such great differences, in relation to the amount of gases in the character of the blood flowing to and from the organs (the arterial and venous blood), if all the carbonic acid of the blood flowing to the right side of the heart were formed gradually and alone throughout the whole extent of the blood-column passing from the left to the right side of the heart through the capillaries? No further probabilities need be adduced to prove that the parenchyma of the organs is the seat of the formation of carbonic acid, as we obtain the most convincing proof of the correctness of this view from the admirable investigations of G. Liebig. Although many points may be susceptible of improvement in the method of experimenting adopted by G. Liebig, the main results must continue unaffected. We have already noticed the most essential facts which have been brought to light by these inquiries. We would here only repeat that the carefully prepared frogs' muscles absorb oxygen and exhale carbonic acid so long as their irritability or contractility lasts, and that the latter is lost in irrespirable gases; and finally, that a muscle completely deprived of blood continues to maintain this interchange of gases so long as it retains its contractility. We have here, therefore, not the mere representation, but the perfect expression of the respiration of an organ of a higher animal without blood, and even without any special air-passages; the interchange of gases and the formation of carbonic acid originate here directly from the organ from which the carbonic acid is otherwise conveyed to the atmosphere by various and indirect means (and necessarily through the blood and lungs). As, moreover, these experiments show that the muscles cannot retain their activity without an access of free oxygen, at all events a large portion of this gas must, after its absorption by the lungs, be conveyed in a free state through the blood and the walls of the capillaries into the muscles. The blood is, therefore, quite as well adapted to convey to the muscles the free oxygen necessary for the accomplishment of their functions, as to carry off the carbonic acid formed by this function; the *first interchange of gases* is, therefore, effected in the *parenchyma of the organs* themselves, or if we regard the interchange of gases between two different media as the process of respiration, the first act of this process—the first interchange—is effected between the parenchymatous juice and the blood in the capillaries."

This, it will be seen, is a point of vital importance for forming a correct idea of the respiratory process. It shows at once the incorrectness of that opinion which holds that certain elements of the food, the sugars and the fats, are taken into the blood as oil is poured into a lamp, simply to be burned, without ever being assimilated; and demonstrates that the carbonic acid, whatever be the mode of its formation, is only transported by the blood, and makes its first appearance in the tissues themselves. But animals shut up in an atmosphere of hydrogen or nitrogen still exhale carbonic acid. The carbonic acid, therefore, does not, even in the tissues, originate by means of a direct oxidation, but by a catalysis with decomposition.

"We have already endeavoured to show," says Lehmann (vol. ii. p. 476), "that wherever oxygen and organic matters enter into combination, they do not at once yield carbonic acid and water as the products of their combustion, and that these simple oxides (as in putrefaction and decomposition) are often only simply separated from the oxidized body, without the organic body being entirely destroyed as in combustion."

It follows, then, that the theory of respiration proposed by Lavoisier, and retained, with some modifications, till within a recent period, represents but very imperfectly the changes which actually take place in the living organism. The absorption of oxygen and the exhalation of carbonic acid are the beginning and the end of the process; but between these two phenomena there is no direct connection. They are separated by a long series of intermediate combinations and decompositions, to which the term combustion cannot be applied with any degree of propriety. The true character of the development of animal heat is very happily expressed by Lehmann when he says that it is to be regarded, not so much as the *object* as the *consequence* of most of the chemical movements in the organism.

"Animal heat," he remarks (vol. ii. p. 480), "has, perhaps, been brought too prominently forward in the consideration of the metamorphosis of animal matter, so that it may almost have appeared as if a great number of the animal processes were accomplished solely for the purpose of generating heat in the living body. When we inquire into the objects accomplished in the organism, animal heat acquires a special significance from the fact that most of the higher animals, however they may otherwise differ, are endowed with a power of compensation, which is so carefully adapted to each that even the most different external or internal relations are scarcely able to produce the slightest fluctuations of temperature. The conclusion which we might be led to draw from this fact, in reference to the importance of animal heat for the vital functions, is certainly somewhat shaken by the consideration that many of the so-called cold-blooded animals, from the agility of their movements, the nature of their food, their respiratory equivalents, the energy of their growth and nutrition, in short, from the amount of their metamorphosis of matter, are not so far different from mammals and birds as to establish the necessity of this high degree of temperature for the maintenance of life, and the energetic performance of the most essential vital functions." * * * "All the admirable investigations which have led us to recognize an internal connection between respiration, certain nutrient matters, and animal heat, have afforded us a deeper insight into the vital processes; and hence it is no poetical imagery to connect the life of respiring beings, in reference to their production of heat, with the process of combustion. Animal heat does not, however, on that account, occupy a higher place than every other phenomenon and every other result which is manifested in the active living organism; at once an effect and a cause, it proceeds, as in combustion, from processes on which it exerts a favourable reflex action; it is only one, but not the highest link, of that immeasurable series of phenomena which constitute the true substance of corporeal existence, and is in certain organisms nothing more than the inevitable consequence of the chemical processes of the animal organism—nothing more than the final result of a movement regulated by definite laws."

No one can read attentively Lehmann's work without being persuaded that, while occupying his attention, the subject of physiological chemistry is in good hands. He possesses the two most essential elements of success; for he is thoroughly acquainted with the deficiencies of the science in its present condition, and understands, also, the nature of the questions which it is called upon to solve.

J. C. D.